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than by the first method. If he should go still further, and object to calling a day foul unless at least .05 of an inch of rain fell, and proceed to verify the above predictions accordingly, the percentage of success would rapidly approach zero. By disregarding this evident truth, Prof. H. A. Hazen has, in his letter on p. 322 of the last volume of *Science*, involved himself apparently in great confusion.

Mr. Rotch and the writer have during the last year published statements showing that local predictions issued from the Blue Hill Observatory for longer periods in advance than those issued by the Signal Service for this vicinity have had a higher percentage of success than the predictions of the latter. Some of these statements were copied in the notes of foreign meteorological journals, and were prominently referred to in an article by Dr. Klein.

In September, 1887, letters were received from Professor Hazen in which he referred to these statements, and said that our supposed higher success was 'all moonshine,' and was entirely due to our methods of verification. Moreover, he said it was unfair to verify predictions made for Massachusetts by the Boston record alone, and proposed that he and the writer should try together predicting for Boston alone. This seemed eminently fair, and the writer agreed to it; but, to make sure that both had a clear understanding of the meaning of the terms to be used, definitions of the terms 'fair weather,' etc., used by the writer in making predictions, published by the Associated Press of southern New England, were sent to Professor Hazen. He materially modified these, and sent the following definitions and rules. The temperature rules are omitted.

**PLAN FOR WEATHER AND TEMPERATURE PREDICTIONS AND VERIFICATIONS AT BOSTON AND WASHINGTON (ALL VERIFICATIONS TO DEPEND ON THE OBSERVATIONS [TAKEN TRI-DAILY AT BOSTON]; PREDICTIONS TO BE MADE AT OR BEFORE 2 P.M., TO HOLD FROM MIDNIGHT TO MIDNIGHT).**

*Prediction: Fair Weather.*—Successful: if fair three times; cloudy, fair, clear in any order; and any cloudiness less. Failure: if cloudy twice in any order; cloudy, fair, fair in any order, and any cloudiness above; a drop of rain.

*Prediction: Threatening.*—Success: if cloudy twice in any order; cloudy, fair, fair and any cloudiness above; rain .01 or less. Failure: if fair three times; cloudy, fair, clear in any order; and any cloudiness less; rain over .01.

*Prediction: Rain.*—Success: rain at any time over .01. Failure: rain .01 or less and any cloudiness.

Predictions were begun according to these rules, and the writer sent Professor Hazen a prediction during each day in October except on Sundays. Professor Hazen has correctly given these predictions, with the corresponding weather at Boston, on p. 323 of the last volume of *Science*. If any one will take these tables, and carefully verify the predictions in accordance with the above rules, he will find that sixteen of the predictions in Column 1, which represent the Blue Hill predictions, were verified, that is, sixty-four per cent of the whole; while only twelve of No. 2 (Professor Hazen's) were verified, or forty-eight per cent of the whole. This excess of sixteen per cent for Blue Hill apparently did not suit Professor Hazen, and he proceeds to obtain from Professors Russell and Upton other definitions and rules for verifying fair, threatening, and rainy weather; and, finding that these give a higher per cent for No. 2, he omits entirely to give his own rules. The writer likes Professor Upton's scheme better than that of Professor Hazen, only his predictions were not made in accordance with such a scheme. The predictions sent to Professor Hazen were not made to be verified in detail, but only to agree with his rules; and it so happened, that, while the writer was predicting with Professor Hazen, he was also predicting for the Boston papers; and when he predicted in these, "rain followed by fair weather," or *vice versa*, he merely wrote on Professor Hazen's card "rain," because, according to Professor Hazen's rules, any rain of over .01 of an inch was to be accounted success. Hence it is seen to be manifestly unfair to verify them by other rules.

According to the definitions sent to voluntary observers by the Signal Office, a fair day is one on which less than .01 of an inch of rain or snow (melted) fell, while a foul day is one on which .01 of

an inch or more fell; and the writer was recently told by one of the predicting officers of the Signal Service that this was virtually the method used in the official verifications.

At Blue Hill this definition has been adopted, and hence the predictions are exactly comparable with those of the Signal Service. For October the Blue Hill predictions thus verified gave a percentage of success of eighty-five, while the Signal Service predictions only gave fifty-eight per cent for this vicinity. In both cases Sundays were omitted. Professor Hazen knew how this percentage was obtained, and yet in his letter to *Science* he writes as if it were a surprising thing that the same predictions should give eighty-five per cent when two things were considered, and only sixty-four per cent when three things were considered, in the verification.

H. HELM CLAYTON.

Blue Hill Observatory, Jan. 4.

**American Microscopes.**

In my letter to *Science* (x. No. 252) in regard to American microscopes, I stated that my opinion in regard to them was based upon the examination of those brought to me by students. I hoped thus to avoid the appearance of claiming to have made an exhaustive examination of all forms of American microscopes. I regret that I did not make an express disclaimer.

Dr. Prudden has placed me under obligation by his very courteous letter in *Science* of Dec. 23, which calls attention to Grunow's new stands. Dr. Prudden's surmise that I was unaware of Grunow's recent work is correct. It is with much pleasure that I now learn that he is endeavoring to meet so admirably the demands of professional biologists and the needs of students.

Mr. Edward Bausch considers me unjust, if I do not misinterpret his letter (*Science*, Dec. 23). He appears to me to have overlooked that I wrote only in regard to microscopes suitable for biological, and particularly histological work. I have heard that the elaborate American stands were favorites with amateurs, but in regard to that point I expressed no opinion. I believe, however, that the increased demand for what is known as the continental stand is due to the rapid growth in numbers of those who use the microscope as a professional instrument, and to the extensive introduction of laboratory work in histology as a part of the course of instruction in our colleges and medical schools.

In regard to the Harvard microscope, Mr. Bausch may recollect, that, when he first came to consult me, I then urged upon him the advisability of frankly imitating one of the Zeiss stands. This advice he decided not to follow. At the time of his second visit I think that I again expressed to him the same advice. I also counselled him to make certain essential and some minor alterations. He made all of the latter, none of the former, if my memory is correct. He subsequently sent me a stand and two objectives to test. In reply I wrote the opinion which he has quoted in his letter, and which I see no occasion to alter now, but am compelled to append a remark for my own justification. The remark is, that I have since then examined a number of the Harvard microscopes brought to me by students. The stands have been of fairly good workmanship, but the objectives I have found, by conscientious examination, to be not infrequently of inferior quality, and most decidedly not satisfactory. As far, therefore, as my experience enables me to judge, I still feel disinclined to bestow the commendation upon these special American microscopes which I am ready to give to some of their foreign competitors.

My letter was not intended to impugn the honesty of the American manufacturers of microscopes, and I do not wish to do so at all. I do wish to call attention to the fact that their policy has been to supply instruments, which, however suitable for certain persons, are not as satisfactory for the work of the professional biologist, the medical practitioner, and of students, as are certain of the European microscopes.

It is to be hoped that Professor Ryder's interesting letter will bring about the result he suggests, of having a competent committee take up the consideration of the best attainable microscope. For my own part, I feel much pleased with a German stand of quite new model, which I purchased last summer. After using it a good deal, I have little change to wish for in it. If it should please others equally, it may be considered to represent an advance towards

the ideal anticipated by Professor Ryder. As to the duty on scientific instruments and books, probably the scientific men of the country object unanimously. One of them said to me once, "When I express myself mildly, I call it a disgrace to the country and an outrage on science." *Science* might accomplish a valuable service by collecting and publishing expressions of opinion on this part of the tariff from some of the leading scientific men of the country. Would not a petition to Congress to abolish the duty on scientific instruments and books in foreign languages find many and distinguished signers?

CHARLES SEDGWICK MINOT.

Boston, Dec. 23.

#### Arkansaw and Kansaw.

WHERE can one find a copy of the law fixing the pronunciation of 'Arkansas'?

As I remember the phraseology, it runs thus: "Each *a* shall be sounded as *a* in 'father,'" or, "Each *a* shall have the Italian sound of *a*, as in 'far,' 'father,' etc." This would require us to pronounce the name 'Ar'-karn-sar' (not dwelling on the *r*) or 'Ah'-kahn-sah.' Mr. Hill pleads for consistency in pronunciation (!): is he consistent? How can he be when he gives three distinct values for the *a*'s in 'Arkansas'? If the last *a* should be sounded as *aw* in 'law,' consistency would require us to say 'Aw'-kawn-saw.' The final '-saw' hardly represents the common pronunciation of early writers, as there was a great diversity. We find, 'Acansæ,' 'Acansas,' 'Accances,' 'A Kancea,' 'A Kansaes,' 'A Kanse' (Marquette's 'A Kansæ,' Jefferys' 'A Kansis'), etc. All of these will appear hereafter in 'Indian Synonymy,' when published by the Bureau of Ethnology.

Though not a New Englander, I propose to adhere to 'Ar-kan-sas' when speaking the English name, and 'A'-kan-sa' when I use the Indian one, though I run the risk of being thought inconsistent.

As to 'Kansas,' how can Mr. Hill say that 'Kansaw' was the early Anglo-American pronunciation, when he gives Long's 'Konza' (i.e., 'Kon'-zay' or 'Con'-zay') as an approximation of the true pronunciation? 'Kan'-ze' (*n* a vanishing nasal, *a* as in 'father,' *e* as in 'they') is the name of the Kansa, Kansas, or Kaw tribe, as given to me by the Indians themselves. This agrees with what I have gained from cognate tribes, the Omahas, Ponkas, and Osages. The early French forms of the name are 'Canzé' (1722), 'Cansez' (1701?), 'Cances' (1702), 'Canzez' (1758), 'Canzas' (1774), 'Kancas' (1753), 'Kansé' (1722), 'Kanses' (1702). Early Anglo-American forms are 'Cansa' (1705), 'Kansæ' (1741), 'Kanzas' (1695), 'Kansez' (1761), 'Kances' (Pike), and 'Kar'-sa' (LEWIS and CLARKE, *Discov.* 1806, p. 13).

The Quapaws or Kwapa say that they were originally part of the Kansas, and the former are the same as the Akansa. Query: was 'A Kansa' or 'A Kanze' ('A-Kan-sæ,' COXE, 1741) derived from 'Kanze'?

There has been a tendency on the part of some Americans to change the Indian *a* as in 'father,' and *e* as in 'they,' to *aw* as in 'law.' Thus: 'U-ga'-Khpæ' ('Oo-gókh'-pah') is now 'Quaw-paw,' or 'Quapaw'; 'Wa-zha-zhe' ('War-zhar'-zhay'), or 'Osage,' is given as 'Was-ba-shaw,' 'Pan'-ka' ('Pahn'-kah'), as 'Pün-caw,' and 'U-ma'-ha' ('Oo-mah'-hah') as 'O-maw-haw.' So 'Arkansaw' and 'Kansaw.' I protest against such cacophonies, which are neither English nor Indian. When the regular Indian pronunciation of a word cannot be retained, let us use one that is euphonious English.

J. OWEN DORSEY.

Bureau of Ethnology, Washington, D.C., Jan. 3.

#### Cheyenne.

MR. WILSON says (*Science*, Nov. 11, 1887, p. 239) that *Shah-ee-ai loo-hah*, said by the Dakotas to the first Cheyennes met by them, means 'you have painted yourselves red.' Its real meaning is, 'you have or possess (*loo-hah*) a Cheyenne (*Shah-ee-ay-lah*). *Lu-ha* (*loo-hah*), 'you have' or 'possess,' is from *yu-ha* (*yoo-hah*), which cannot be used as an auxiliary in forming the perfect tense (for which there is no exact Dakota equivalent). 'You have painted yourselves red' must be expressed by *shah-neel-ch'ee-yah'-pee*, in which *shah* is 'red;' *nee-ch'ee*, reflexive pronoun, second person; *yah*, causative; and *pee*, the plural ending. J. OWEN DORSEY.

Bureau of Ethnology, Washington, D.C., Jan. 3.

#### The Eskimo Ring-Finger.

WE found the habit of wearing finger-rings quite general among the Eskimo of Point Barrow during the two years we spent among them (1881-83). These rings are generally made of brass, rarely of silver, and it was quite natural to suppose that they learned the fashion from American whalers. The ring, however, is always worn on the middle finger, and indeed received its name (*katukgle-rñ*) from *katukglñ* ('middle finger'), corresponding to the Greenlandic *kiterdlek* (literally 'the middle'). This circumstance was supposed to be merely accidental, especially as the word used in modern Greenlandic for ring does not indicate any particular finger, meaning simply 'the thing which belongs on a finger' (*agssangmio*).

The use of rings is not mentioned, as far as I can tell, by any writers who have described the Eskimo (though *agssangmio* occurs in Kleinschmidt's Dictionary), and every thing favored the belief that the fashion was merely local at Point Barrow and in Greenland (and possibly elsewhere), and had been learned after they had come in contact with civilized people.

I was not a little surprised, therefore, when I had an opportunity of consulting the earliest Eskimo dictionary (that of Paul Egede, published in 1750), to find given as a derivative of the word *kiterdlek* (which, by the way, appears in the form *katerdlek*, decidedly nearer the Point Barrow pronunciation), *katerleraut* ('a ring:' "annulus, quia Groenlandi annulum in medio digito gestare").

Whatever may be the fashion nowadays in Greenland, it is quite plain that in Egede's time the Greenlanders, like their more unsophisticated cousins at Point Barrow, not only wore the ring on the middle finger, but named it from that finger.

Moreover, the word for 'ring' in the Mackenzie River dialect (*kpitep-klopox*) indicates a similar fashion in that region. Such a coincidence in widely separated branches of the same race could hardly be the result of accident. Nor is it easy to see how any circumstances of environment could have affected such a trifling matter as which finger a ring should be worn on.

Evidently, therefore, before the Eskimo had separated into their present branches, they ornamented their hands with rings, which they wore on the middle finger, and not on what the white race have for ages considered as the ring-finger.

The question of the position of the ring-finger may appear, as I have called it, a trifling matter; but I think I have shown it to be a link in the chain of evidence connecting the different branches of the Eskimo race, and, as such, worthy of consideration.

JOHN MURDOCH.

Smithsonian Institution, Jan. 4.

#### Queries.

22. WASP-STINGS.—I have often, from childhood to the present time, heard the assertion that while one holds his breath it is impossible for him to be stung by a wasp. I have till recently always dismissed the assertion with the same smile that I have the statement that swallows hibernate in the mud, or that Friday is an unlucky day. My only reason now for asking place in the columns of *Science* for a question concerning it is the persistent assertion, made by a gentleman of the highest intelligence, whose opinions and judgment are of recognized value in scientific as well as other departments of thought, that the statement is true. Unfortunately, my own experiments have only been with wasps that were rendered somewhat torpid by cold weather, and count for nothing either way. I cannot learn that similar claims are made in regard to bees or hornets; nor can I learn, from those who make them in regard to wasps, whether it is claimed that the act of holding the breath renders one's skin impervious to the wasp's sting, or whether it in some way changes the nature of the virus or of the sensitiveness of the flesh to it. The assertion simply is, that any one may, while holding the breath, handle the liveliest and most able-bodied wasps with perfect safety, and also without after-pain or ill effect from any efforts of the wasp made while respiration was suspended. Can any readers of *Science* prove or disprove these assertions, and, in case they are sustained, give any theory whatever in explanation?

C. H. AMES.

Boston, Dec. 28.